April 17, 1956

Dear Francis and Betty:

It isn't the mails, just your correspondent that's so slow. We were actually rather annoyed that you hadn't written use sooner-- and then I realized how long a time had elapsed before I could get to answer your titillating note. We envy you your trip, but even more so your capacity to organize and enjoy them; I'd be exhausted before I could even get under way.

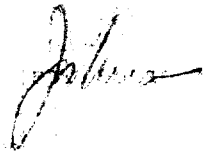
Not much new here that I could put in a few words; I've been spending most of my time cleaning up older issues, and trying to write about them: it will give you some idea how I've been spending my time if I tell you that there are right now eight mss. in press; I have just one more to go— the abortive transductions of motility — and I can feel a free man again, for a while; this peak is all just neglect, for I did little of it the past two years.

Our lab's been remodelled, and this just finished so we can settle back into our routines; since Thanksgiving, we'd been parked in the Bacteriology Building here as guests while the men swarmed (too vigorous a word, perhaps droned would be better) around here. We have little more acreage, but laid out much better. Anyhow, that was quite a drain on energy; it was a wonder we could keep going at all.

As to recombination (it occurs to me I have subconsciously held off until after you finished lecturing about it), I finally decided to put out at least a little note about the conjugal pairs (trusting you know the essentials of that, namely that Hfr and F- pair side by side, then separate; both exconjugants are viable; recombinants segregate out of the F- exconjugant clone; the patterns speak for the same elimination process [pre- or postzygotic?] as did the diploids and other crosses) though the cytological details are still incomplete. Jacob [vice Hayes] and I are still quarreling, not very vehemently I hope, about just when the eliminations occur; quite possibly, we've both been right (and wrong) and the breaks are prezygotic [but I insist at a fixed position in any given genotype] and the losses may occur at either time. I don't think these details are of very fundamental importance. Lately, I've been dawdling with a variety of different Hfr's, each of which shows a distinctive segregation (i.e. presumably elimination) pattern when crossed with standard multiple marker F- stocks. I'd been working with UV-induced Hfr's, which are not hard to get with a suitable selection procedure; meantime, the Jacob's come out with a coincidental report on spontaneous Hfr mutants. He seems to believe that <u>all</u> fertility of F+ stocks is due to Hfr mutation, but the evidence is still not altogether convincing, and it sounds like a typical Gallic generalization (cf. Monod's story, now abandoned, that all constitutive [basal] lactase activity is due to constitutive mutants]; the proposition turns out to be hard to defeat, since some Hfr mutants of course do occur, since fertility is rather sensitive to environmental factors, e.g. aeration and especially because crossing of F+ x F- is rather hard to get at, with its low frequency. Alan Richter is turning up some evidence that Skaar's motilization effect, viz. the isolation of F- from F+ passed through soft agar, is selective after all, but depends on a rather small difference in motility of F+ and F-, while other genes can modify motility quite drastically. This is relatively uninteresting; Dave and I had thought or hoped that the effect would be like fast growth pn kappa.

One of my very best students is a Japanese lad from Misima, Tetsuo Iino; we've just sent in a paper to Genetics on the control of phase variation in Salmonella. The H_2 gene exists in two states, independent of its antigenic specificity, one is epi- the other hypostatic to H_1. How the state is maintained, we don't know, are just working on methods of quantitating the rate of variation; we do know only that it is locally determined, and not say by the cytoplasm. H_1 is or is not manifest according to the state of H_2; it has not heritable alternation of states of its own.

I know you don't want to think of coming back, but all good things to come to an end. I was sorry in a way you weren't interested in the NSF job; I hope the fact that Vernon Bryson had been holding it before did not depreciate its importance for a more experienced researcher,



and I had somehow thought you might like the political life. The NSF is definitely on the way up, and faster than ever now, and it is vitally important that a few (other) good people make the sacrifice of helping keep it righteous. I hope I don't sound stuffily as if I have anything to do with such appointments; I found I was only one of very many people who had recommended you to NSF for that particular job. I only have to ask myself whether I would even consider such an (unlikely) opportunity myself to realize why you might would turn it down, but one can sometimes hope. I did know you were conscientiously concerned about social angles of science and its support, and whoever they do get, won't do nearly as well (from our point of view, not yours).